Sept. 11, 1959

Dear Avrion:

To yours of the 2d; many thanks for your remarks.

I am interested to hear that you're staying in Edinburgh, or at least not taking on the London job.

Gus Nossal arrived a couple of weeks ago, and is organizing his lab. very effectively despite some difficulties-- his permanent space won't be ready until Jan. or Feb. It would have been hopeless to get you going this soon; however, I think that just about any time after say March 1960 we'd be delighted to have you. Let me know what I might do to help.

Just to get something underway, Gus and I are checking on the 'adherence phenomenon' -- lymphoid cells which make anti-flagellar antibody include some to which the bacteria adhere. Not all the cells provoke adherence, and there is some question as to the specificity of the reaction (e.g. is it related to just one component of the H antigen?) While we run down specific cross-reacting components, in further tests of the scope of antibodies produced by single cells, we can check this item too. Have you any ideas about specific adherence?

No, we've not proceeded at all with our staff appointment-- waiting for just the right man to present himself. I will be making further inquiries.

Do you think that specific haptens really add so much to your analysis? Certainly one would prefer to use them, on general grounds. I would take care to use as carrier one of the proteins of the host itself if you mean to minimize irrelevant reactions. It would be amusing to know whether a soluble hapten could induce and maintain tolerance.

Where is Boyse's work published-- I hadn't seen it yet. I would like to see the following control: immunize an F1 and inject these cells into an F1 together with some P1, unimmunized cells. The stimulation you referred to might be a nonspecific (humoral?) incident of the occurrence of a graft vs. host reaction. If it is so indirect as to involve some competition against host lymphoid cells, this control still wouldn't work.

We're well installed now in the medical center, and the facilities are splendid; as a small department we have to lean very heavily on Biochemistry, which is just as well. Mel Cohn is set up about 3 doors down the hall; his work with Lennox on diverse Ab yields from single cells is certainly very impressive. They are also reporting that a fair proportion of their ABF cells are not plasma cells but lymphocytes, which again points up some differences between their phenomenon and Gus's. I think it is still quite likely that the duration of exposure to the antigens is the critical difference.

Robert White has sent a very pretty picture of differentially stained cells producing one or another Ab; his experiments are also fairly short term.

As ever,